On October 6, 1821, Victor Emmanuel I. instituted the first Italian public chair of higher physics, and in November Avogadro was appointed to it. Owing to political troubles the post was suppressed two years later, and Avogadro, after a brief interval, was given a post at the Regia Camera dei Conti, where he continued with undiminished activity his cherished studies. In January, 1832, Carlo Alberto restored the chair Avogadro had occupied, but appointed to it the famous Cauchy, who, it will be remembered, had been compelled to leave the Collège de France and his seat in the National Institute of France owing to the Revolution of 1830. Cauchy had been elected, amidst a storm of indignation, to the seat in the institute vacant by the expulsion of Monge, and he was now in his turn suffering the vicissitudes of fickle fortune.

Towards the end of 1833 Cauchy left Turin to take charge of the education of the young princes at Prague, and Avogadro was again appointed to the position for which he was so well fitted. He remained professor of physics until 1850, when he resigned owing to advancing age, and was succeeded by his favourite pupil, Felice Chiò. His scientific work was continued almost to the end of his life; and he died at Turin on July 9, 1856, having nearly reached the age of eighty.

Avogadro's memoirs, numbering some forty or fifty, appeared in the *Journal de Physique*, the *Annales de Chimie et de Physique*, *Brugnatelli's Journal*, and other periodicals. Among his works was a huge compilation of 3700 pages, in four volumes, on physics, which was published at the expense of Carlo Alberto. His principal studies, as we have seen, related to the physical properties and the internal structure of bodies, and the law—that equal volumes of gas contain the same number of molecules—to which his name is attached perpetuates his memory. Ampère independently enunciated the same doctrine; but in point of priority Avogadro's claim is beyond dispute.

He was a man esteemed in private life for his kindness, affability, and sincerity. His habits were noted for their simplicity, and he was as modest as he was learned. The year following his death a marble bust in his honour was placed in the University of Turin. In the Scuola Professionale at Biella is another bust, with the inscription—

I GAS A PARI CONDIZIONI
DI PRESSIONE E DI TEMPERATURA
IN EGUALI VOLUMI CONTENGONO
UN EGUAL NUMERO DI MOLECOLE.

Last year a project was formed to celebrate the centenary of the enunciation of his law, but I do not know whether the scheme was carried through or not.[1] The above details have been obtained from the short biography of Avogadro by Alfonso Cossa, for a copy of which I am indebted to the kindness of Prof. Naccari, the present occupant of the chair of physics at Turin.

EDGAR C. SMITH.

Hong Kong, October 23.

Characteristic Röntgen Radiations.

IN the September number of *The Philosophical Magazine* there appears a paper by Prof. Barkla, in which are tabulated the results of some further experiments on characteristic (fluorescent) Röntgen radiations. It appears that some of the elements of high atomic weight give "two lines in the fluorescent spectrum." Optical analogy suggests that there may be some simple relation connecting these spectral lines. Such a relation can, indeed, be readily obtained, and I should like to be allowed to direct attention to it.

In a paper on the production of characteristic Röntgen rays (Roy. Soc. Proc., 1911), I showed that for the elements from Al upwards (which gave characteristic rays in series K) the radiation they emitted could be defined, indirectly, in terms of their atomic weight. It was shown that the radiation characteristic of an element of atomic weight w is the same (when tested by penetrating power) as the most penetrating of the Röntgen rays emitted from an anti-kathode bombarded by kathode rays of velocity equal to kw, where $k=10^8$.

Thus the radiation characteristic of Se would be emitted by kathode rays of velocity $7\cdot92\times10^9$ cm./sec. ($w=79\cdot2$), that characteristic of Cu by kathode rays of velocity $6\cdot36\times10^9$ cm./sec. ($w=63\cdot6$), and so on. Such kathode rays as would produce a characteristic Röntgen-radiation may be conveniently termed equivalent kathode rays. Thus we can define a radiation the λ/ρ of which is known, in terms of the velocity of the equivalent kathode rays, and so indirectly in terms of the atomic weight.

Prof. Barkla tells us that Bi gives out (in series L) a radiation of very nearly the same penetrating power as that from Se in series K (the actual values of λ/ρ are 19·0 and 18·9). From this it follows that the equivalent velocity for Bi (series L) is the same as for Se (series K), *i.e.* $7\cdot92\times10^9$. Thus, if we adopt series K for our standard, we can say that Bi behaves (for series L) as though it were an element of atomic weight 79.

The actual atomic weight of Bi is 208, and it behaves (for series L) as if its atomic weight were $w'=\frac{1}{2}(w-50)=79$; thus the two possible spectral lines for Bi are defined in terms of the two equivalent kathode rays of velocities $2\cdot08\times10^{10}$ cm./sec. and $7\cdot9\times10^9$ cm./sec.

The expression

$$w'=\tfrac{1}{2}(w-50)$$

holds fairly well over the greater part of the range studied by Prof. Barkla, as the following table shows.

Element	w	w'	λ/ρ (calculated)	λ/ρ (observed)
Sb	120·2	35·1	375	435
I	126·9	38·5	298	306
Ba	137·4	43·7	248	224
W	184·0	67·0	35·0	33
Pt	195·0	72·5	25·5	27·5
Au	197·2	73·6	23·8	25
Pb	207·1	78·6	20·0	20
Bi	208·0	79·0	19·1	19

The calculated values of λ/ρ have been obtained by interpolation of w' in a w, λ/ρ graph.

The observed value of λ/ρ for Sb is almost certainly too high, since in some experiments with Sb (August, 1910, Proc. Camb. Phil. Soc.) I found that the equivalent velocity was about $3\cdot6\times10^9$ cm./sec. (compare w' above). The agreement otherwise is as good as could be expected.

In conclusion, formulæ of the type to which attention has just been directed ($w'=A.w+B$) may possibly prove useful to experimental investigators in suggesting new "lines" and indicating where to look for them.

R. WHIDDINGTON.

St. John's College, Cambridge, November 16.

A Suggested Reform in Palæobotany.

IN a paper in the *Annals of Botany* for October (pp. 903–7, text figures) I brought forward conclusive botanical evidence that the Cretaceous fossils from the Amboy Clays of North America, hitherto known as *Ophioglossum granulatum*, Heer, are not only not Ophioglossums, but are not ferns at all, and belong to the widely distinct family of Gymnosperms, in the genus Pinus.

This result, in itself of not much importance, forms the text of a general recommendation to palæobotanists, which is as follows:—In the interests of the sciences of palæobotany, geology, and botany, I "urge that the lists published by palæobotanists should be printed in two forms, and that the names of species of leaves, stems, &c., of which there is a reasonable security of determination, should be differentiated from those in which there is no guarantee at all that the actual nature of the plant has been discovered. Any tri-nomial system is cumbrous; but those who publish on fossil plants might print their names in type of two kinds, which would indicate which species are doubtful. I should like to suggest that, instead of using italics or ordinary capitals as is usual in printing the names of species and genera, such doubtful plant-impressions should be printed in Gothic lettering. This would indicate that our knowledge about them is mediæval—of the Dark Ages—and would further save the inconvenience of tri-nomials, while it would indicate immediately the difference between the established and the doubtful determinations. As information accrued about a

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[1] See NATURE of October 26, p. 557.—ED.

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specimen it could easily be transferred to the clear Latin italics.

"Thus the transference of Ophiogloſsum granulatum, Heer, to *Pinus granulata* (Heer) would indicate that an exceedingly doubtful determination had been replaced by one with some scientific basis. Any worker in another branch of science, seeing O, granulatum, in Gothic, would be warned at least to look into the grounds for the determination for himself before he—let us imagine—used the record for his stratigraphic work in correlating horizons, or in writing up the early history of the Ophioglossaceæ, when he would otherwise assume that the living genus was represented in Cretaceous times in the Ambay Clays of North America. This is merely an illustration of what is very widely spread in fossil botany; but it may serve to give point to the general proposition that the time has come when it would be of real service to the science to attempt a conscientious distinction between valuable and doubtful determinations, and that Gothic lettering might give us an easy indicator.

"The need for this is all the greater, because the results of palæobotany touch so many other fields of research, in animal palæontology, geology, and palæogeography, as well as botany itself. Workers from these other fields are seldom able to estimate the evidence that they are taking to build into their own work, even had they the time to go into the details; and thus a single error gets widely disseminated. Often it is not entirely the fault of the one who originally described the fossil, for he may say in his text that the nature of the specimen is doubtful, and that, in default of better evidence, he gives a certain name with hesitation. That name, however, once given, is quoted and put into lists without being in any way distinguished from the rest; and the results are detrimental to the advance of true knowledge in every way. It is no solution to call every leaf, as some conscientious workers do, 'Phyllites': different things, however doubtful, must have distinct names, and the use of Gothic characters for the very doubtful ones would greatly tend to 'create confidence' in the science of palæobotany."

MARIE C. STOPES.

The Unit of Momentum.

DOUBTLESS all teachers of experience are agreed that in dealing with the measurable quantities considered in mathematics and physics there is great difficulty in giving to students a clear idea of the quantities measured unless a definite name be given to the unit in terms of which measurement is made.

Until the word "radian" was introduced, circular measure was a stumbling-block to all beginners in trigonometry; the sentences "the circular measure of this angle is 1·7" and "arc over radius equals 1·7" make little impression on the untutored mind; but the sentence "this angle contains 1·7 radians—the radian being rather more than 57°" is as easy of comprehension as "the value of this is 2¼ guineas—the guinea being 21 shillings." So in electricity, without the names ampere, volt, ohm, &c., the learner would be—as he was thirty years ago—lost in hopeless vagueness.

In mechanics we have given names to almost all the units; why not give one to the unit of momentum? I venture to suggest for general use the names which I have myself made use of for many years, and have found helpful in inducing clear thinking on the part of my students; they are analogous to the names of the units of work.

Thus the work done by a force 1 lb. acting for 1 ft. is one ft-lb.

The momentum produced by a force 1 lb. acting for 1 sec. is one sec-lb.

The work done by a force 1 poundal acting for 1 ft. is one ft-poundal.

The momentum produced by a force 1 poundal acting for 1 sec. is one sec-poundal.

The work done by a force 1 dyne acting for 1 cm. is one cm-dyne (erg).

The momentum produced by a force 1 dyne acting for 1 sec. is one sec-dyne.

Similarly we may speak of a sec-ton and a sec-tondal.

The advantage of the above names is that they emphasise the essential distinction between momentum and kinetic energy, viz. that one measures what I may call the time-effect of a force and the other its space-effect; or, to put it differently, when we know the momentum of a moving body we know *how long* a given force must act to produce the motion, and when we know its kinetic energy we know *how far* the force must act. This point of view seems to me far more desirable than the suggestion to the beginner that momentum is mv, or "mass × velocity"; such a suggestion he must either receive in deadly apathy or he must worry his brain with the impossible task of trying to conceive how mass can be *multiplied* by velocity: his teacher may try to slur over the difficulty for him by a word-juggling substitution, and say momentum is the *product* of mass and velocity; but then, what is a product? In the mathematical sense of the word it is the result of multiplication; in the non-mathematical sense it is anything resulting from or produced from certain others; and in this latter sense kinetic energy is just as much the product of mass and velocity as momentum is. So, also, the time-honoured but vague phrase "quantity of motion" could be used to express kinetic energy with quite as much aptness as to express momentum.

Using these names, we would teach—

"The momentum of m lb. moving at v f.s. is mv sec-poundals, or mv/g sec-lbs.; its K.E. is $\frac{1}{2}mv^2$ ft-poundals, or $\frac{1}{2}mv^2/g$ ft-lbs." "The momentum of m grams moving at v c.s. is mv sec-dynes; its K.E. is $\frac{1}{2}mv^2$ ergs (cm-dynes)."

F. R. BARRELL.

The University, Bristol, November 7.

I AM in the habit of saying, "the amount of momentum is 12 in C.G.S. units" or "the amount of momentum is 12 in engineers' units." The use of such a complex name as "gram-centimetre per second" would be absurd. Now Mr. Barrell has made an excellent suggestion; the names sec-dyne with all students, and sec-pound with such students as use engineers' units, are not only short and easy to remember, but they keep before the student the fundamental fact that force is time rate of change of momentum. It seems to me, however, that at the start we must define momentum as mv, and boys must from the start learn that they are no longer multiplying mere numbers. I never found that a boy had any difficulty if his master did not create one. A boy knows at once that if a distance of 100 yards is passed over in 20 seconds there is an average speed of 5 yards per second; he has no difficulty in dividing space by time, but his teacher may create great confusion in his mind if he philosophises about it.

JOHN PERRY.

Fish and Drought.

IN my letter on "Fish and Drought" published in NATURE of November 23 there is an error, due, without doubt, to a mistake in my MS., which may confuse the reader. In the tenth line from the bottom of the right-hand column of p. 108 the word "north" occurs; it should read "south."

It has been pointed out to me by friends who have read the letter that the final paragraph is rather difficult to follow. With your permission I beg to take this opportunity of rectifying its expression, as follows:—

In conclusion, I think that the observations above recorded show that the material of geological formations need not necessarily have been "laid down"; it may have been produced *in situ* like the mud in the ditch round the Park of Marchais, and that the enclosure in it of animal remains may have been in some cases due to a voluntary act of self-inhumation, undertaken, perhaps usually, with a view to self-protection. They also show that two neighbouring strata, the one carrying abundance of life and the other being destitute of it, may nevertheless be contemporaneous in date and conterminous in locality of formation.

J. Y. BUCHANAN.

November 26.

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